This seemed conclusive, but I naturally turned for further corroboration to the passage in the *Quarterly Review* referred to.

Judge of my surprise when I found, on the preceding page (p. 208), the following passage, which the *Athenæum* reviewer, had evidently neglected to read when he declared that the person who wrote the book was not known: —

"Our first impression, on taking up the volume, was, that, as the subject of the Arctic regions had become one of the fashionable topics of the day . . . some hanger-on of Paternoster Row had contrived, with the help of Egede, Fabricius, and the interminable Cyclopædia of Dr. Rees, to hash up a fictitious voyage to Davis's Strait, in order to gratify the eager appetite of the public, and at the same time to 'put money in his purse.' Recollecting, however, that the log-book of the ship 'Thomas,' of Hull, in which this voyage is stated to have been made, was within our reach, we turned to it, and found that *Bernard O'Reilly, Esq., was not, as we suspected, a phantom conjured up for the occasion, but that there actually was a person of this name, in the capacity of surgeon, on board that ship*" [the Italics are mine].

The process of "evisceration" referred to by the *Athenæum* reviewer then begins with great ferocity — too great, it seems to me, even for such a ridiculous book as it is. This, however, only proves, what is easily seen from reading the book, that it is quite worthless from a scientific point of view, and evidently the work of what we should call a "crank" nowadays, not that it is not the work of the man whose name appears on the titlepage.

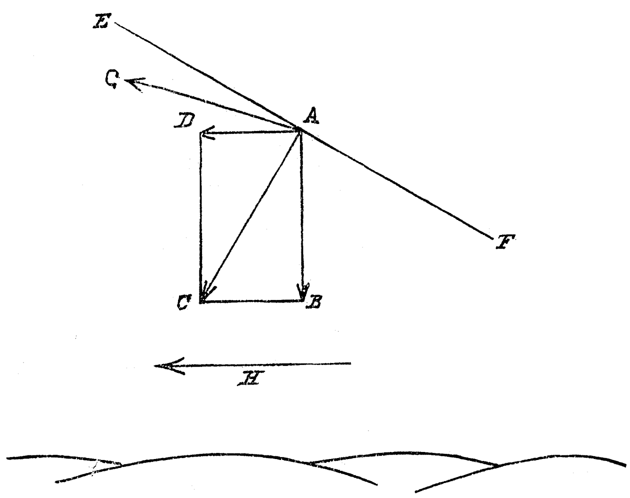
If the *Athenæum* reviewer has no further evidence to submit in regard to the authorship of the book, I do not see how we can doubt that the book is genuine, even though it is not authentic (which is quite another matter), and that it was perfectly proper to insert the title in a bibliography of such extended scope as Mr. Pilling's work. In fact, it would have been a mistake for Mr. Pilling to omit the book from his list, in spite of all its glaring absurdities.

JOHN MURDOCH.

Smithsonian Institution, Feb. 18.

The Soaring of Birds.

OWING to the remoteness of my present situation, I have but just seen the query of Mr. Kent. The point which he makes is a good one. I see also that I made a slight misstatement in my previous article. In the first place, as before remarked, we have the force AB due to the weight of the bird, and the force AD due to the



excess of velocity of the wind over the velocity of the bird. These two forces may be combined into the resultant AC. This resultant is resisted by the force CA, due to the resistance of the air acting on the wings of the bird as he wheels in circles about the point A. The force CA is therefore proportional to a function of the velocity with which the bird moves in describing these circles. The greater the velocity of the bird, the greater the force. Now, this velocity which he is capable of attaining with regard to the wind is dependent solely on the absolute velocity of the wind with regard to the earth, and can never be more than twice as great; i.e., when he is moving with the velocity of the wind and in the opposite direction.

Now let the velocity of the wind be x, and the excess of its velocity over the mean linear velocity of the bird (with respect to the earth) in the same direction as the wind be a, then the mean linear velocity of the bird with regard to the earth will be $x—a$. We will suppose that the velocity of the wind is such that $CA=AC$: the bird will therefore continue to revolve about the point A, which will consequently be its mean position. It must, of course, be remembered, that, while these forces are in equilibrium, the bird is slowly drifting over the earth's surface in the same direction as the wind. Its mean position would therefore describe a horizontal line with respect to the earth.

Now suppose the velocity of the wind (x) to increase, while its excess over that of the bird (a) remains the same: AC will therefore remain constant. But the velocity of the bird with regard to the earth ($x—a$), and also his absolute velocity with respect to the surrounding air, have increased, and therefore CA has increased also. Accordingly, the bird will be carried above and to the right of the point A. In the mean time the bird is drifting rapidly towards the left, in the direction of the wind: he will therefore describe a path lying in the same general direction as the line AG, *Q. E. D.*

W. H. PICKERING.

Los Angeles, Cal., Feb. 11.

A RECENT number of *Science* (xii. p. 267) gives an account of a paper by Mr. G. K. Gilbert, containing a theory of the soaring of birds, which traces this phenomenon to the advantage gained by the bird in gliding to and fro between contiguous horizontal layers of a horizontal wind, moving at different rates. The theory presented is said to have been anticipated by Lord Rayleigh (*Nature*, xxvii. p. 534); but it seems to me to rest upon what is, apparently at any rate, quite a different assumption from that which Lord Rayleigh made.

Mr. Gilbert imagines a bird gliding to windward in the lower of two contiguous layers, and traces the changes which his relative velocity will undergo if he first pass into the upper layer, then turn in it, then move to leeward, passing into the lower layer, and finally complete a cycle by turning to windward. He concludes, that, after the completion of the cycle, the bird's velocity will have increased by twice the velocity of the upper layer relative to the lower, frictional resistance being left out of account. This result he obtains by assuming, that, after turning, the bird's velocity, relative to the medium in which he turns, will be the same as before; in other words, that during the turn his velocity relative to the earth will change by an amount equal to twice the velocity, relative to the earth, of the medium in which the turn is made; the change being an increment in the turn to windward, and a decrement in the turn to leeward. Of course, in accounting for the phenomenon of soaring, some assumption must be made as to the bird's power of regulating the magnitude and direction of the force exerted upon him by the wind. But it should be a reasonable one, and, if not evidently so, should be justified. Mr. Gilbert's assumption does not seem evidently reasonable, and yet he does not even refer to its having been made.

In another recent number of *Science* (xiii. p. 31), Professor Pickering has shown that in a uniform horizontal wind the phenomenon of soaring is quite consistent with the law of the conservation of energy, provided frictional resistance is not too great, but he does not show how it may be accomplished. Lord Rayleigh, on the other hand, has stated that a uniform horizontal wind certainly cannot help us to explain this phenomenon. With so emphatic a statement from so high an authority, one is fearful of rushing in where angels fear to tread in attempting an explanation on this hypothesis. Nevertheless I venture to submit to your readers the following considerations, showing, I think, how soaring may occur in a horizontal wind which has no differential motion.

The force exerted by a horizontal wind on a bird may clearly be inclined upwards; for the wind, striking the lower surface of the wing, is deflected downwards, and must therefore have been acted upon by the wing with a downward force. The wind must therefore have exerted on the wing an upward force. What the exact direction and magnitude of this upward force will be, will depend upon the velocity of the wind relative to the bird, the wing area, and the ingenuity of the bird in adjusting its wings. With a strong wind and a wing area large relatively to the mass of the bird, it

may readily be large enough to have a vertical component equal to the bird's weight, in which case the resultant force on the bird may be horizontal.

Let us suppose, now, that a bird is at any instant moving horizontally, in the same direction as the wind, and with a small velocity relative to the earth. Since the resultant force on him may be horizontal, he may continue to move horizontally with increasing speed. As his speed increases, the velocity of the wind relative to him diminishes, and therefore also, probably, the upward force exerted on him by the wind. Although, therefore, the resultant force on the bird may have been initially horizontal, it will not remain so even for a short time. But it may remain for some time very nearly horizontal; for, as the magnitude of the relative velocity diminishes, its inclination to the normal to the plane of the wings will diminish also. During that time the bird will move slightly downwards, and his velocity will increase. When his velocity has become so great, and therefore the velocity of the wind relative to him so small, that the resultant force on him begins to have a direction differing markedly from the horizontal, let the bird wheel and steer upwards to windward. Let us suppose that in wheeling he maintains his velocity relative to the earth as well as his elevation. Then, starting upwards with a considerable velocity, he will clearly be able to rise through a certain height before his velocity has been reduced to its initial value. Let him then wheel again, and he will now be in a position to repeat the cycle with the same starting conditions as before. Whether soaring has been accomplished or not, will depend on whether or not the height gained when moving to windward is or is not greater than that lost in moving to leeward.

To determine this, consider first the downward part of the cycle. Let W_1 be the mean vertical component, and W_2 the mean horizontal component, of the force exerted by the wind on the bird's wings. Let R be the mean resistance to the relative motion of bird and air (due to friction, etc.), which in this case helps the bird. Let w be the weight of the bird, h the height through which he falls, and d the horizontal distance he traverses. Then the work done on the bird by the vertical and horizontal forces will be $(w - W_1)h$ and $(R + W_2)d$ respectively (we may treat R as a horizontal force, because the path is nearly horizontal). Let H be the energy expended immediately or ultimately in the production of heat. Then the kinetic energy gained by the bird on the downward motion will be —

$$(w - W_1)h + (R + W_2)d - H.$$

During the upward motion against the wind, the mean velocity of the wind relative to the bird will be much greater than during the downward motion with the wind; but while the direction of the relative velocity during the downward motion was upward, during the upward motion it is downward. It seems reasonable, therefore, to suppose that the upward force exerted by the wind may be made by the bird the same as before, and may have, therefore, the same components, W_1 and W_2. Let R' be the mean resistance of the air due to friction, etc. R', in this case, impedes the motion of the bird. Let w, as before, be the bird's weight; and let h' be the height through which he rises, and d' the distance traversed horizontally. Then the work done by the bird against the forces acting on him will be —

$$(w - W_1)h' + (R' + W_2)d'.$$

If the bird wheels when the energy expended on the upward motion is just equal to that gained on the downward motion, he will be ready to begin his second cycle under the same starting conditions as his first, and we shall have, for determining the height to which he has risen, the equation —

$$(w - W_1)h + (R + W_2)d - H = (w - W_1)h' + (R' + W_2)d',$$

from which it follows that the gain of elevation

$$h' - h = \frac{Rd - R'd' + W_2(d - d') - H}{w - W_1}.$$

Since during the upward motion against the wind the mean value of the velocity of the air relative to the bird is greater than in the downward motion, R' will be greater than R. But the bird can so steer his course as to give his path a greater inclination to the horizon than his downward path had: hence d' may be made smaller than d; and thus $Rd - R'd'$ may, by good steering, be made positive. Also, d' being less than d, $W_2(d - d')$ will be positive. If these two quantities together are greater than H, $h' - h$ will be positive; and if, finally, the increase of energy represented by the elevation $h' - h$ is greater than the inevitable waste during the turns, the bird will have increased his elevation during the cycle.

It seems to me possible, therefore, for a bird to soar in a uniform horizontal wind; because, by falling slowly in the motion to leeward, he allows the wind to do a large amount of work on him, and, by rising rapidly in moving to windward, he may regain his former level without having to do so much work against the wind. If it is possible, the bird's path must clearly be a spiral about a line rising in the direction of the wind, not about a vertical line; and this agrees exactly with observed fact. J. G. MACGREGOR.

Dalhousie College, Halifax, N.S., Feb. 5.

Some Habits of the Omahas.

IN the article entitled "Some Habits of the Omahas," on p. 60 of *Science* for Jan. 25, was a slip of the pen, which I wish to correct. Both Omahas and Ponkas, who speak the same dialect, call the wild honey "bee-dung." The term "bee-gum" was given me in 1872 by a Ponka, my interpreter, who stated that it was not the old name. My Omaha informant, Samuel Fremont, does not wish incorrect statements credited to him. J. OWEN DORSEY.

Takoma Park, D.C., Feb. 13.

Sawdust Explosion.

I ENCLOSE you a cutting from the Ottawa *Journal* in reference to what is called a "sawdust explosion," as it is a somewhat unique phenomenon. Last winter one occurred in the Ottawa River opposite this city, near the place referred to in this article, which broke up the thick ice over a large space. The river-channel is deep, but it is filled with a great accumulation of sawdust from the large mills just above. This sawdust generates immense quantities of marsh-gas, and once in a while something seems to start it up suddenly in large volumes. These striking the under side of the ice with great force, burst it up in the manner here described. This is why they are called "explosions." The gas is never ignited.

"Mr. J. de St. Denis Lemoine, sergeant-at-arms of the Senate, was blown up in a sawdust explosion on the Ottawa River, Saturday, Feb. 9, 1889, at midnight. He escaped with a wetting, and will not snowshoe to Gatineau Point again. It was a jolly party of gentlemen who left the city Saturday evening for a tramp on the ice-bound Ottawa. It included Messrs. Riddington, Lemoine, R. Fleming, J. Travers Lewis, J. W. Pugsley, Charles Elliott, Laurence Taylor, W. Middleton, Bogert, G. A. Henderson, and some others.

"The snowshoers headed direct to Gatineau Point, where an enjoyable time was spent. They started for home shortly before midnight. Mr. Riddington led the way, the snowshoers following in Indian file at a distance of about ten feet apart. The leader cautiously picked his way, because an ominous crackle here and there gave warning of proximity to cold waters running a few inches beneath.

"Matters went well for a time, until, some little distance below the Rideau Falls, suddenly the snowshoers were startled by a terrific explosion. An instant later they saw Mr. Lemoine hurled in the air, and as suddenly fall back into a mass of broken ice. It was only the work of a moment to grasp the sash of Mr. Lemoine and haul him on to the firm ice, not much the worse for his partial wetting. There would have been a funeral had the sergeant-at-arms been in the middle of the explosion.

"Mr. J. Travers Lewis had a narrow escape. Fortunately he stopped for a moment to fix his snowshoe-strings, and, had he continued in the footsteps of Mr. Lemoine, would also likely have experienced a sad fate.

"The snowshoers say that in their opinion the sawdust question has been solved." ROBERT BELL.

Ottawa, Can., Feb. 13.